

VALID UNTIL SYNONYMIZED, OR INVALID UNTIL PROVEN VALID?
A RESPONSE TO DAVIS (2004) ON SPECIES CHECK-LISTS

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The recent publication of taxonomic authority lists for the non-marine mollusks of France (Falkner et al., 2002) and of north and north-west Europe (Falkner et al., 2001) has elicited critical comments from Davis (2004). I have been involved in both works, for the French list as instigator and editor, for the European list as a provider of supraspecific nomenclature (Bank et al. 2001). The purpose of the present response is to place both lists in perspective, to justify the editorial decisions taken, and to defend the rationale behind the scientific decisions made.

On Checklists and Taxonomic Authority Lists

Taxonomic authority lists or checklists are as old as the science of systematics. However, unlike many other groups of animals, large or small, mollusks do not have a comprehensive catalogue of species, or even of names. The last academic attempt to list the Recent mollusks of the world was Tryon and Pilsbry's *Manual of Conchology*, now on average 100 years old. With an accumulated load of perhaps 500,000 names and a synonymy ratio that is matched probably only in butterflies, the compilation of a global mollusk checklist is not a small task. The result is that we do not even know whether the number of valid named Recent species of mollusks is on the order of 50,000 or 100,000, an uncertainty that is persistent throughout Recent and fossil biota but is seen as "particularly problematic" for mollusks (Hammond, 1995).

Information technology has suddenly made it much easier to compile and update species catalogues that reflect changes in knowledge and thus taxonomic instability, whereas, simultaneously, a growing corpus of legal texts and other documents, such as Red Lists, demand authoritative lists of names that will change little over time. Recently published regional checklists emphasize one or the other of these two approaches. For instance, Turgeon et al.'s

(1998) list was compiled to provide an authoritative reference for U.S. federal and state conservation texts, and it emphasizes stability and established knowledge over scientific inquisitiveness and controversial opinions.

In Europe, there has been a long tradition of national checklists of non-marine mollusks (see, e.g., Bruyne et al., 1994; Manganelli et al., 1995; Kerney, 1999), but no continent-wide list has been published since Westerlund's catalogues of the 1870–1880s. In 1998, proposals were made to issue a taxonomic authority list of the land and freshwater metazoans of geographical Europe. With funding from the European Commission, Fauna Europaea was formally initiated in 2000 for a period of four years, and Ruud Bank was chosen to be the "Group Coordinator" (in Fauna Europaea parlance) for the gastropods. At the onset, it was estimated that there would be on the order of 3,000 valid molluscan terminal taxa (species and subspecies; see below), and it was also recognized that, because of the chaos caused by the Nouvelle Ecole, the French fauna was the major stumbling block in compiling a list of valid taxa. I then decided to contract Gerhard Falkner and Theo Ripken, both with an extensive knowledge of the French fauna, to produce a taxonomic authority list for France. The result is the Falkner et al. publication appeared in March 2002, but the species list had already been made available for the CLECOM [Check List of the European Continental Molluscs] catalogue (covering the countries of northern and north-western Europe), involving Falkner and Ripken as co-authors and published the year before on the occasion of the World Congress of Malacology in Vienna in August 2001. In turn, the CLECOM catalogue became the core of the Fauna Europaea checklist, released electronically in October 2004 (Bank, 2004). Although the three products are embedded within each other and have complementary scientific contents, they differ in format in addition to geographical scope. The French checklist comes

with 110 pages of endnotes that justify taxonomic or nomenclatural decisions, or report new faunistic records. The CLECOM and Fauna Europaea lists do not have associated notes, the former is available on paper and electronically, and the latter only in electronic form.

I should like to emphasize that taxonomic checklists are only as good as the quality of the science that is behind them. Davis' (2004) criticisms focus in particular on the poor taxonomy of hydrobioids in the French and CLECOM checklists. I do not disagree. But the culprits are the systematists themselves, past and present, who have been and are establishing new taxa without proper qualifications and comparisons. The French checklist was about to go to press when Bernasconi (2000) published a work on the *Bythinella* of southwestern France in a non-peer reviewed publication, with the description of seven new species. As editor of the list, I agreed with Falkner that the new species should be given the benefit of the doubt and be listed as valid until synonymized. Falkner et al. (2001, 2002), CLECOM and Fauna Europaea thus list 37 valid species of *Bythinella* from France, but the French checklist emphasizes (endnote 78, page 86): "Bernasconi's results and our own will have to be tested by using molecular characters. In the meantime, segregating morphotypes at the rank of species will better meet the needs of mapping and conservation programs that motivated the compiling of the present list." I believe that highlighting the problems does a better service to science than suppressing them. I agree with Davis that one may, or even should, view the listing of 36 species and subspecies of *Bythiospeum* in Germany with disbelief. Davis' comment "I do not know of any molecular or detailed anatomical study that has looked at variability within and between populations to infer possible genetic breaks in taxa of *Bythiospeum*" is, in my view, not a criticism of list compilers but a criticism of those who add to the confusion by establishing still more new species based on inadequate character analysis and comparisons. To make a comparison with North America, I would suggest that a future edition of Turgeon et al.'s *Common and scientific names of aquatic invertebrates from the United States and Canada* should list *Physella hemphilli* D. W. Taylor, 2003, and *Physella winnipegensis* Pip, 2004, as valid, unless these nominal species have been synonymized; by doing so, Turgeon et al. would not

be making a judgement on Taylor's (2003) or Pip's (2004) work. Checklists are simply reflections of the state of the art, and one should not "shoot the messenger" if the news is not good.

On Bourguignat and Revalidation of the Nouvelle Ecole's Nominal Taxa

Few personalities in the world of malacology have elicited so much criticism, and even hatred, as Jules-René Bourguignat. It is fair to recognize that Bourguignat had a sharp discriminating eye for characters, that his knowledge of the literature was immense, and that he had a network of correspondants that channelled large amounts of valuable material to him from all over the western Palaearctic (Kuiper, 1969). However, Bourguignat also had very personal views on what deserved to be ranked as a species, and he developed an undefensible system whereby he would rank as "species" specimens that would be diagnosable by three characters. This, in combination with a self-infatuated personality and personal attacks on his competitors, invited the wrath of established and influential European malacologists. Both Crosse and Kobelt used the journals they edited, *Journal de Conchyliologie* in Paris and *Nachrichtsblatt der Deutschen Malakozoologischen Gesellschaft* in Frankfurt respectively, to build a sanitary cordon around Bourguignat and his followers, the self declared "Nouvelle Ecole". Bourguignat retaliated with a gifted pen and ridiculed his enemies in "*Lettres Malacologiques*" and other polemic writings (Bourguignat, 1882). The two camps being at war with each other, Bourguignat's school would not listen to any, justified or unjustified, criticism and went on unchecked to establish thousands of new nominal species.

By the end of the 19th century, Locard recognized no less than 1850 valid non-marine mollusk species in the French fauna (Locard, 1893), among them no less than 506 species of unionids (versus eight now regarded as valid at the species level). The uncompromising attitude of the "Nouvelle Ecole" was mirrored in the other camp by the rejection *en masse* of Bourguignat's works and species. In the decades that followed, any species named by Bourguignat, Locard, Mabille, or Caziot, to name just a few, was *a priori* suspected to be synonymous of an earlier "classical" species, that is, a species recognized by British or Ger-

man authors. The question then asked was not "Is this a valid species or a synonym?", but "Which species is this a synonym of?" The first decades of the 20th century were thus a period of massive synonymization; to ridicule the insignificance of the species established by Locard, Coutagne (1929: 16) even created the word "locardies", a parallel to the "jordanons" of botanical literature. Germain started his career by co-authoring two papers with Locard, but later became the principal instrument of the synonymization of the Nouvelle Ecole's nominal species. Germain's two volumes of the *Faune de France* (Germain, 1931) represent the culmination of bringing the French non-marine fauna into harmony with its time. Because of the chaos caused by the Nouvelle Ecole's oversplitting, Germain's "normalization" was received with much relief and his new synonymizing was gladly and uncritically accepted by his contemporaries and followers. It was not until the 1970s that the French fauna received new, critical attention from malacologists from the Netherlands, Germany, Italy, and Spain. In particular, the Rijksmuseum in Leiden (today Naturalis) made southwestern Europe its area of excellence, resumed comprehensive field work, and started to critically re-examine the systematics of the land snails from France, Spain and Portugal based on solid population-based species concepts and using anatomical, and, later, molecular data. This led to the resurrection of several nominal species from the graveyard of synonymy, e.g., *Abida occidentalis* (Fagot, 1888), a local endemic from the central Pyrenees, resurrected from the synonymy of *A. pyrenaearia* (Michaud, 1831) (Gittenberger, 1973), and *Ceruellia aginnica* (Locard, 1894), broadly distributed in southern France, resurrected from the synonymy of *C. virgata* (da Costa, 1778) (Clerx & Gittenberger, 1977). When revalidating *Trichia phorochaetia* (Bourguignat, 1864), endemic to the Grande Chartreuse and Vercors regions of the French Alps, Winter (1990) commented: "Notwithstanding the good description and figures provided by Bourguignat (1864), the species was placed by both Hesse (1921) and Germain (1930) in the synonymy of *Trichia villosa*, no doubt because of Bourguignat's reputation."

It became clear in the 1980s and 1990s that, among the many superfluous names produced by the Nouvelle Ecole, not everything was a synonym, and in fact Bourguignat and his followers had named some perfectly valid spe-

cies, often local endemics from the Alps, the Pyrenees or the Mediterranean region. What Falkner and Ripken did when working up the French checklist was to critically re-examine as many nominal species as possible, based on the original collections, including types, of Bourguignat (in Muséum d'Histoire Naturelle de la Ville de Genève), of Locard (in Muséum National d'Histoire Naturelle, Paris), and of Caziot (in Muséum d'Histoire Naturelle de Nice), an approach that, surprisingly, no one had done systematically before. In the overwhelming majority of the nominal species they re-examined, they confirmed earlier accepted synonymies. However, this work also revealed a number of taxa that they suspected represent valid species: rather than pushing these into synonymy against the available evidence, they decided to give the benefit of the doubt to these taxa. For instance, the French checklist thus revalidated *Oxychilus colliourensis* (Locard, 1894) and *O. adjaciensis* (Caziot, 1904), based on historical as well as newly collected material. It also tentatively listed as valid, for example, *Limax granosus* (Béranguier, 1900) and *Milax ochraceus* (Béranguier, 1900) because of their distinctive anatomy, despite their not having been found in the last 100 years (but also, admittedly, they have not been searched for at the type localities). As the editor of the French checklist, I agreed that "giving their chance" to *Limax granosus* and *Milax ochraceus* as potentially valid species was more likely to lead to hypothesis testing and falsification, than continuing to obliterate them as doubtful synonyms (and then, as synonyms of what?).

To conclude on Bourguignat and the Nouvelle Ecole chapter, I would like to make a comparison with another (I am afraid, also French!) malacologist of the 19th century who has been the subject of much controversy on the other side of the Atlantic, I mean Constantin Schmaltz Rafinesque of course. It has been said that Rafinesque was his own worst enemy, and the same could be said of Bourguignat. Their published works were so controversial, their personalities were so unconventional, that they became ostracized to the point of suppression. For a long time, malacologists in the United States ignored Rafinesque's names, while European malacologists who dared declare any of Bourguignat's species as valid were stigmatized. Admittedly, Rafinesque and Bourguignat wreaked havoc on the systematics and nomenclature of North American freshwater

mollusks, and western Palearctic land snails, respectively. But the rules of nomenclature demand that names and works are evaluated on a case-by-case basis. Rehabilitating *Trichia phorochoaetia* as a valid species is not a rehabilitation of its author, personality or method. After 130 years of blanket ostracism of Bourguignat, let's acknowledge that some valid species were indeed named by Bourguignat, even if such cases fuel incendiary judgements on zoological nomenclature: "In other sciences the work of incompetents is merely ignored; in taxonomy, because of priority, it is preserved" (Michener, cited by Gould, 1992).

On Subspecies

Continental European, especially Germanic authors, have a long tradition of systematics formally recognizing subspecies for discrete geographical variations within species, and the CLECOM and French checklists, as well as Fauna Europaea, clearly belong to this school. As regional checklists recently published in Europe vary in how they treat geographical variation, some background information is useful to place the Germanic tradition in perspective.

(1) For decades, many authors have used indiscriminately the concepts of "variety" and "subspecies", which is reflected to this day in the *International Code of Zoological Nomenclature* that regulates how varietal names can be rescued when applied to subspecies. In the 19th century and well into the 20th century, the concept of "variety" was used to designate any kind of variation (size, colour, sculpture), with or without a geographical component, and at any scale (local, regional or global). Taxonomic and nomenclatural practice shifted when evolutionary thought changed our understanding of variation. Taxonomists started to treat species as groups of populations, rather than collections of individuals, and they observed that morphological gaps could occasionally be overlaid with geographical gaps. This approach was first conceptualized in the 1930s by Bernhard Rensch and Ernst Mayr, at the time both working in the Berlin Museum. Mayr's well-known influence on bird systematics was a reflection of the immense impact of his teaching and writing on geographical variation. Less well known abroad is the role of Rensch, who had taken over in Berlin after Thiele, before becoming a professor at the

University of Münster. Rensch had a considerable influence on German evolutionary systematics after WWII and, because he was also a malacologist (see, among others, Rensch, 1937), his impact on German malacology cannot be overestimated.

(2) It certainly is no accident that the study of geographical variation had a much higher resonance in the heart of alpine Europe than elsewhere in Europe. Most of northern Europe was covered by ice during the glacial periods of the Quaternary, and its current fauna and flora could become established there only as the land became free of ice: for example, in the British Isles, nearly all the species that live today arrived there less than 10,000 years ago. This recent colonization has two consequences: first, there are no endemic species in the British fauna and flora; and, second, there is no discernible geographical variation among the British populations of even broadly distributed species. If British (or Scandinavian) authors do not recognize subspecies, this is not because Rensch was wrong, but rather because there are no subspecies *within* the British (or Scandinavian) fauna.

By contrast, glaciations have created in alpine Europe a mosaic of refuges. Whereas the major valleys were occupied by glaciers, there emerged archipelagoes of unglaciated territories, for example, slopes facing south, nunataks, thermal areas, and populations isolated in these refuges had time to genetically diverge between two interglacial cycles. Superimposed on the complex topography and climatology of the mountain areas of southern Europe, the glacial cycles had the effect of breaking down territorial and genetic continuities, generating the many highly localized species and subspecies that today characterize alpine and Mediterranean Europe. The Alps thus naturally became the playground of malacologists applying the concepts of Mayr and Rensch's evolutionary systematics.

How we translate geographical variation into classification is not just an academic exercise in nomenclature, but is embedded in evolutionary biology and has consequences for management and conservation. Subspecies based on morphology (just as well as species) are hypotheses of genetic relationships between specimens considered representative of natural populations. In this respect, the allopatric subspecies of evolutionary systematists are the "Least-Inclusive Taxonomic Units" (LITU) of phylogenetic systematists (Pleijel & Rouse 2000). It does not really mat-

ter, and it is in fact a matter of personal choice, whether one wants to call these terminal taxa species or subspecies, and whether one bases such decisions in a Phylogenetic Species Concept or a Biological Species Concept. For that matter, Kottelat's (1995) "Pragmatic Species Concept" is not lurking far behind. I believe that George Davis and I do not stand far apart on this issue, and I agree that taxonomic principles inspiring the checklist compilers could have been made more explicit. In fact, the whole issue broadly falls within the hotly debated subject of taxonomic ranks. Isaac et al. (2004) have recently discussed how changes in species concept, rather than new discoveries, are leading to raising known subspecies to species level, with consequences on macroecology and conservation biology. What really matters is that these terminal taxa should be seen as biological/evolutionary/management units, rather than the esoteric fancy of a taxonomic splitter.

Davis (2004) defends the view that "it is inappropriate to name subspecies as a convenience and in the absence of well-founded data". None can disagree with him on this point, although we may disagree on what constitutes "well-founded data". However, I believe

that Davis' criticisms are not aimed at the right target and do not do justice to the state-of-the-art of European non-marine molluscan taxonomy. The checklists compilers are not working in a nomenclatural *terra nullius*, and the names are in fact already out there in the 250 years of accumulated literature on European non-marine mollusks, however brilliant or pathetic, modern or outdated. In the checklists being discussed, the list compilers did *not* name any new subspecies, but they *recorded the use* of subspecies names in the latest authoritative publications on the subject. There is already a considerable body of literature on the geographical variation and the distribution of subspecies of continental European land snails. In this respect, the French checklist only reflects the state-of-the art of that existing literature. For instance, the subspecific taxonomy of *Chilostoma zonatum* (Studer, 1820) is based on Forcart (1933), that of *Abida secale* (Draparnaud, 1801) is based on Gittenberger (1973), and that of *Clausilia rugosa* (Draparnaud, 1801) is based on Nordsieck (1990). One of several taxonomic areas where the French checklist innovates is unionid systematics below the species level. In that family, taxonomic stability had been reached several

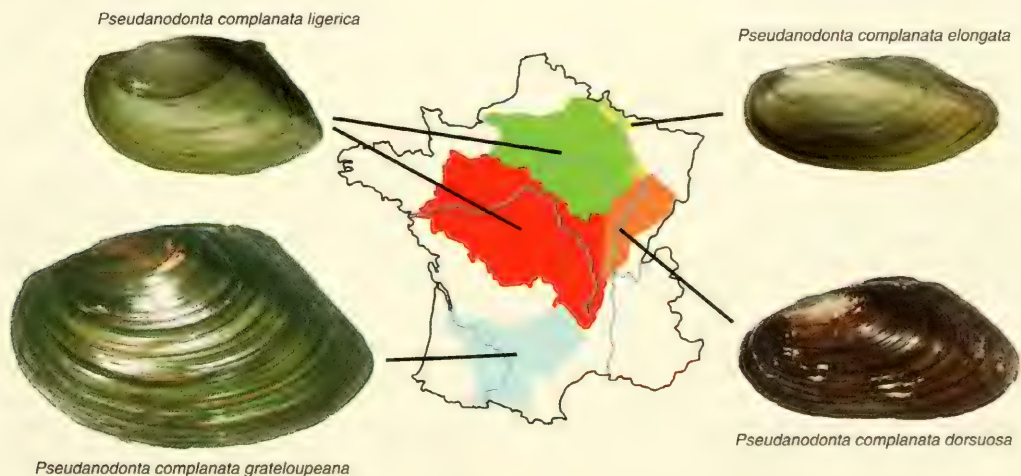


FIG. 1. Variation in European unionids has been historically disconcerting and difficult to analyze, because it includes a high within-population component and a more discrete geographical, between-population, component. By segregating discrete subspecies, the recently published French checklist hypothesizes that the morphologically recognizable forms from major drainages do have biological significance that must be taken into account into biodiversity inventories and management schemes. For instance, hypothetical populations reinforcements should avoid translocations of individuals between populations of *Pseudanodonta complanata* from the Moselle (a tributary of the Rhine), Seine-Loire, Garonne, and Saône (a tributary of the Rhône) drainages. [Copied from Bouchet, in Falkner et al., 2002: 12].

decades ago at the species level, with *all* of the hundreds of Nouvelle Ecole names ending up in synonymy. However, recent genetic work provides grounds for recognizing "subspecies" within these morphologically defined taxa. For instance, Badino et al. (1991) commented that a phenogram of genetic distances between populations of *Unio elongatulus* C. Pfeiffer, 1825 [now *U. mancus* Lamarck, 1819], and *Unio pictorum* (Linnaeus, 1758) showed an "almost perfect arrangement of populations according to the hydrographic basins". Recognition of "subspecies" within the unionids of France reflects the hypothesis that different hydrographic basins are inhabited by distinct genetic stocks, as evidenced by discrete morphological differences between basins (Fig. 1). As editor of the French checklist, I agreed with Falkner's decision to highlight such discrete differences by using subspecific names, so that ecologists and geneticists could test and challenge them, rather than merging all regional variants into a broad, apparently uniform pool. I had earlier (Bouchet et al., 1999) advocated subspecies to be an appropriate level for establishing lists of protected taxa under the European legal instruments.

Regional Species Checklists: What They Are Not

There are also points where I do agree with Davis, in particular classification, when he writes "Check lists should not be a vehicle for promoting the reconstruction of phylogenetic history or promoting a particular phylogenetic hypothesis" (Davis, 2004: 230). I agree that regional checklists are not the place to produce elaborate, finely dissected classifications. A classification using subfamilies, tribes and subgenera may be necessary when the purpose is to catalogue hundreds of species of Enidae from the Middle East and central Asia, but it certainly is not necessary when there are only six species of Enidae in the French fauna. This criticism of the inappropriateness of overly elaborate classifications in country or regional checklists probably is most founded in the case of hydrobioids which, in addition to the necessity of addressing their classification in a global context, are also undergoing a phase of profound re-evaluation (e.g., Wilke et al., 2001). The usefulness of national and regional checklists is because their compilers have an intimate knowledge of a usually highly fragmented local literature,

both in space and time, dealing with taxonomic status and distribution of the terminal taxa. (Almost 64% of the 3,000 references in the French check-list are papers, pamphlets and books published in France.) However, the body of literature dealing with higher classification is of an entirely different nature, has no geographical borders, and is fast changing. In the currently very active phase of reevaluation of the phylogeny of the mollusks, any classification is certain to become rapidly outdated (Bouchet & Rocroi, 2005).

It should also be recognized that, in the case of the European fauna, regional species checklists are faced with special difficulties, because of the huge synonymy load, conflicting or parallel taxonomic schools, and centuries of accumulated literature, opinions, and mistakes. For fewer than 5,000 terminal taxa, there may be somewhere around 50,000 nomenclaturally available names. Under these circumstances, these European regional species checklists cannot be and were not intended to be taxonomic *revisions* nor comprehensive nomenclatural compilations, in which every name is listed and/or every taxonomic opinion is supported by facts and references. As such, the checklists are not themselves standard, falsifiable research products, although they do synthesize such research results.

Taxonomic "Authority" Lists: Authoritative to Whom? The Tyranny of Users

The discussion that is now taking place in *Malacologia* about the French and the CLECOM checklists raises the issue of the acceptance of such checklists by the rest of the malacological community *and* users in general. These two categories of users may in fact at times have diverging interests, and this is where problems of acceptance may arise. Just like the *International Code of Zoological Nomenclature*, taxonomic authority lists only work as long as there is a majority consensus to accept them. The International Commission on Zoological Nomenclature does not have a police to enforce violations of the Code, and the general adherence to the *Code* marginalizes those zoologists who might reject the *Code*. In other words, the community of systematic zoologists will only accept those rules that it is prepared to follow. In turn, the International Commission on Zoological No-

menclature is led to propose compromises between consistency, for example, the "priority rule", and established usage, that may inconsistently apply the rules.

Taxonomic "authority" lists face the same kind of dilemma. If they list only those taxa that are accepted by an overwhelming majority (95%?) of systematists, they will be seen as perpetuating the worst conservatism and will give the impression that everything is known and no further research in taxonomy is necessary. If they list nominal taxa that have not yet been properly scrutinized by peers, they run the risk of disseminating instability. However, the role of checklists is not primarily to give a false impression of stability where there is not. When passing judgement on the authoritativeness of a checklist, one should not underestimate the tyranny of non-specialist users whose demand for "stability" is legitimate when it concerns the nomenclature of reasonably understood biological entities, but is not legitimate when it closes the door to progress in knowledge, or even to ambiguity (see, e.g., Dubois, 1998). Likewise, compilers of checklists must sometimes find compromises between scientific rectitude and the expectations of users. I want to illustrate this point by an example involving George Davis' own research.

For nearly 100 years, British and Irish malacologists have argued over the taxonomic status of populations of *Margaritifera* living in the river Nore in Ireland. Whereas *Margaritifera margaritifera* is a soft-water species everywhere it lives in Europe, there are Irish populations in the Nore basin that live in hard water, that have subtle shell differences that led to their segregation as a different species, *Margaritifera durrovensis*. Until the advent of molecular techniques, it remained disputed and unresolved whether *Margaritifera durrovensis* was just a hardwater variant of *M. margaritifera*, or a distinct species. Subsequently, an allozyme study (Chesney et al., 1993) concluded that *M. durrovensis* was just an ecophenotypic variant. As a scientist, I of course accept the results of the molecular study, but as a conservationist I may understand the desire by Irish naturalists to treat the Nore river *Margaritifera* as a "conservation unit". In fact, Chesney et al. themselves noted that "the classification of *M. durrovensis* as an ecophenotype of *M. margaritifera* does not detract from its need to be conserved". However, how does one give special conservation

consideration to a "*Margaritifera margaritifera* Nore basin conservation unit"? Legislators and regulators have answered that question by placing "*Margaritifera durrovensis*" on Annex 2 to the European Habitats Directive (the EU equivalent to the US Endangered Species Act). And they have done so in 1995, that is, *after* the results of the Chesney et al. (1993) study, which were known to the proponent of the listing. I am not, through this example, advocating that it was justified to place the "species" *Margaritifera durrovensis* or even a "subspecies" *Margaritifera margaritifera durrovensis* on a list of protected species. But the fact is that it was listed, despite the advice of scientists (including myself) consulted by their national regulatory authorities. How was this to be reflected in CLECOM? One course of action was to promote scientific rectitude, ignore the name *durrovensis* altogether, and run the risk of being viewed as "irrelevant" or "useless" by the agencies using taxonomic authority lists for management. Another course of action was to promote consistency between regulatory texts and taxonomic authority lists, list *Margaritifera durrovensis* as valid, and run the risk of being viewed as "incompetent" by professional systematists. After much debate among its authors, CLECOM chose a middle course, and listed *Margaritifera margaritifera durrovensis* as a valid subspecies. It would not take much for me to accept that this compromise is eminently disputable. Just as a classification does *not* reflect *all* the relationships between taxa (the tree does), names are, after all, no more than a tag that people – scientists or non-scientists – find convenient to use to designate a biological "entity" or "unit", and communicate about its attributes and properties. Listing *Margaritifera margaritifera durrovensis* in taxonomic authority lists is a convenient way to access information on its conservation status as well as the associated literature, including Chesney et al. (1993).

This example reminds us that taxonomists and compilers of taxonomic authority lists are not working in a sociological vacuum. There is pressure from non-scientists to have *names* to designate management entities, even when these do not correspond to sound biological units. Davis (2004) defends the idea of using "conservation units instead of dubious subspecies". I believe scientists may argue *ad nauseam* in academic journals on whether "conservation units" are concepts that should be preferred over "dubious subspecies". I am

afraid this is an esoteric debate that has lost sight of the sociology of users who prefer to use named entities over "conservation units". As the Irish *Margaritifera* demonstrates, one powerful reason is that only named entities can have a legal status (that may actually promote research on the status of the taxon in question). I believe that when there is a testable hypothesis that such an entity may be treated as a biologically significant unit *and* there already exists a name for it, then we should definitely use that name. When a name does not exist already, I agree that it may be disputable whether it is justified to establish one, and I would defend the view that it depends on the scientific as well socio-historical context. For three generations, malacologists working on the systematics of European land snails have extensively and consistently used trinomial nomenclature, whereas by contrast, the analysis of geographical variation of North American land snails, and/or the way this variation is traditionally expressed through names, has *not* led to trinomial nomenclature.

To conclude, I would like to proselitize on the recently produced checklists of French and European non-marine mollusks. They represent a huge effort of data collation and knowledge consolidation, and they represent the state-of-the-art of species-level systematics by systematists who have a personal opinion on the validity of nominal taxa, rather than perpetuate the state-of-the-art of 30 or 70 years ago. That the current state-of-the-art is challenging so many entrenched usage traditions is a reflection of the health of non-marine molluscan systematics in Europe. That the current state-of-the-art is highlighting so many unresolved problems also reflects the need for more research. This is why I entitled my introductory chapter to the French checklist: Land and freshwater mollusks of France: a new taxonomic authority list, a new start, new perspectives [*«Mollusques terrestres et aquatiques de France: un nouveau référentiel taxonomique, un nouveau départ, de nouvelles perspectives»*]. To close with a dose of humility, I will quote Isaac et al. (2004) in their recent essay on "taxonomic inflation": "Taxonomic uncertainty is ultimately due to the evolutionary nature of species, and is unlikely to be solved completely by standardization. For the moment, at least, users must acknowledge the limitations of taxonomic lists and avoid unrealistic expectations of species lists."

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